MEDICINE.

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SOME REMARKS ON TYPHOID FEVER; WITH AN ANALYSIS OF 129 CASES.

F. C. SHATTUCK (*Boston Medical and Surgical Journal*, cxxi. 221, 1889) analyzes the results of his experience with typhoid fever in the Massachusetts General Hospital during the last three years. He excludes all abortive or otherwise doubtful cases, but includes those patients who entered the hospital in a desperate condition and died within a few days. In this connection he remarks on the occurrence of abortive typhoid fever, questioning how anyone of experience can doubt its existence, and quoting from the literature of the subject to show that typhoid fever can certainly be present even without the slightest elevation of temperature. Of the 129 cases, 11 died, a mortality of 8.8 per cent. In none of these was unusually or continuously high temperature a feature. A table is given showing the state of the bowels, the occurrence of epistaxis, and the existence of splenic enlargement. This table shows that in 51 per cent. there was no diarrhœa, and in 36 per cent. the bowels moved only after enemata. In only 25 per cent. was looseness of the bowels really a prominent feature, a smaller percentage than the text-books would usually indicate. Epistaxis was present in 38 per cent. The author is inclined to consider splenic enlargement less common than usually supposed. He gives a table of the frequency and character of the complications and sequelæ, and calls attention to the fact that there was no instance of perforation among his cases.

In discussing relapse, the author follows the English writers in defining it as a renewal of the typhoid process, irrespective of the time of its onset; as opposed to the Germans, who claim that to constitute a relapse there must have been an interval of apyrexia of varying length. 21 (16.28 per cent.) of his cases suffered relapse, but as one of them relapsed twice, there are 22 secondary attacks for study. In 11 of these the relapse was intercurrent, *i. e.*, occurred before complete defervescence, before the first normal evening temperature; and in 11 others it was consecutive, *i. e.*, developing after defervescence. This fact proves, he thinks, that we are powerless to prevent a relapse, and that errors in diet have nothing to do with it. In the great majority of his cases the second attack started without any change in the liquid diet. Various causes may occasion a return or an exacerbation of the fever, but to produce a relapse there must be a re-infection or auto-infection.

In the 11 intercurrent cases the relapse took place in from the third to the fifth week. In the 11 consecutive cases it appeared after an interval of from one to nine days. 21 of the cases recovered, the average duration of the second attack having been nineteen days, but with an individual range of from eleven to twenty-nine days. The author has excluded from this list those which might be classified as instances of aborted relapse, on account of the difficulty in distinguishing between such very short attacks and some obscure complication. His experience does not bear out the German view that intercurrent attacks are more dangerous than consecutive ones.

In 18 of his 22 cases the fever reached its height on the fourth to the sixth day, and more often on the fifth than on any other day. The sudden fall of the temperature on the eighth to the ninth day, mentioned by Irvine as characteristic of relapse in typhoid fever, the author has not been able to corroborate throughout. In only 6 of his cases did this drop occur on either of these days. In some it was noticed on the sixth, in more on the tenth or the eleventh, and in others the fever terminated by an uninterrupted but gradual lysis.

The treatment was essentially expectant. In 29 per cent. no drug of any kind nor any alcoholic stimulant was given at any time. Digitalis and cocaine were the cardiac stimulants employed when such medicines were indicated. The author was especially pleased with the apparently prompt and marked action of the latter in some cases. Calomel and naphthalin were administered in a few cases to see whether intestinal antisepsis could be procured, but the results were not encouraging. The author used the internal antipyretics a good deal in 1886, but very little in the two succeeding years, and only then when the fever itself appeared to interfere with the comfort of the patient. The knowledge which we now possess renders it probable that it is not the high temperature which is the great cause of death in typhoid fever, but the influence on the nervous system of the alkaloidal poisons produced in the living body. The author quotes the experience of several observers in support of this statement. He is being gradually forced to the conclusion that the method of Brand for the treatment of typhoid fever is the best, and that we must come to it in this country. The statistics which Brand gives, and which Shattuck quotes, show how very much less the mortality is in patients treated by this method.

NAPHTHALIN AND TYPHOID FEVER.

E. SEHRWALD (*Berl. klin. Wochenschr.*, 1889, xxvi. No. 22, 492), after a series of bacteriological experiments on the action of naphthalin on the typhoid bacilli occurring in typhoid stools, comes to the following conclusions:

1. Naphthalin in substance, acting at the ordinary temperature of a room on the microörganisms of putrefaction, of feces, and of typhoid fever, hinders their development somewhat, though only slightly.

2. Enlargement of the surface of the naphthalin powder increases correspondingly its disinfecting power. Persistent shaking of the fluid containing naphthalin acts in the same way.

3. On increasing the temperature to that of the human body the action of the naphthalin on all the varieties of bacteria mentioned is decidedly greater;

and it therefore becomes probable that it is in the form of a gas that naphthalin acts injuriously and destructively on the microbes.

4. Naphthalin, as a gaseous disinfectant, must act more energetically on the aërobic varieties in fluids than on the anaërobic forms. So, also, it will affect more completely those which grow on a solid culture medium than those living in a fluid.

5. The conditions for the action of naphthalin are much more favorable in the intestine than in the reagent glass; since not only does the body-heat produce a powerful volatilization of the substance, but through the peristalsis the intestinal contents are constantly kept in motion and new portions of the fluid brought into contact with the naphthalin gas.

6. Naphthalin added to feces outside of the body diminishes the number of microbes contained in them about one-half. When given internally it reduces the number to about one-third to one-fourth of the original. The quantity then increases, however, to nearly the initial amount, owing to the great increase of microbes which are more resistant to the action of the drug.

7. The bacteria of the typhoid stool are acted on very powerfully by naphthalin, and reduced nearly ten times. Also on those still remaining after the administration of calomel naphthalin acts very injuriously.

8. Since typhoid fever is to be regarded as a mixed infection, naphthalin, on account of its action on microbes, should be given from the beginning of the affection. And since its gas, at the temperature of the body, acts with especial power in hindering the growth of the typhoid bacilli, the therapeutic employment of the drug is still further justified in this disease.

9. Since calomel has a disinfectant action on a portion of the fecal bacteria in typhoid fever, but spares other forms which are more subject to the action of naphthalin, the earliest possible combined administration of the two drugs must seem to be the most rational procedure.

10. The naphthalin introduced into the intestine is absorbed in part, and undergoes a change in the body and is excreted in the urine. The decomposition-products from the naphthalin, which are found in the blood and urine, have no restraining power on the growth of the typhoid bacilli.

The author believes that the results of his experiments fully justify us in continuing the administration of naphthalin in typhoid fever, with the hope that we may exert a favorable action on the disease.

EQUINO-TELLURIC ORIGIN OF TETANUS.

VERNEUIL (*Prog. Méd.*, April, 1889; *Manchester Med. Chronicle*, June, 1889) terminated the reading of a memoir on this subject at the Académie de Médecine with the following conclusions: 1. Tetanus is transmissible not only between animals of the same species, or of different species, but also from man to man, from animal to man, and reciprocally. 2. Contagion between a tetanic horse and man may be direct or indirect, mediate or intermediate. 3. The intermediary agents are all those objects which, whatever may be their nature, come into contact with a tetanic horse and receive undestroyed the virulent element; multiplicity of contacts and objects increases, therefore, the circle of infection. 4. Living beings can serve as organs of transmission without their being themselves affected; but they are then under the menace of tetanus by traumatic auto-inoculation, this being effected as soon as a point of entry is open to the virus. 5. A wounded man can contract tetanus from the greater part of the objects with which his wounds come in contact, but the cases and experimental researches show that the most dangerous is the horse and all articles connected with it, and cultivated earth and some of its products. 6. The earth only possesses a tetanic virulence after contamination with a tetanic horse. 7. Statistics show that the greatest liability attaches to those who are in habitual contact with horses. The topographical distribution of human tetanus is parallel to the equine variety. In our climate the latter, like glanders, is the more frequent. The prophylaxis of human tetanus is, therefore, in the hands of the veterinary surgeon.

ANTIFEBRIN IN EPILEPSY.

TH. DILLER (*Therapeutic Gazette*, June, 1889) reports in detail the results of his employment of antifebrin in nine cases of epilepsy. The drug was administered in doses of four grains three times a day; though in two cases the amount was doubled.

He sums up his experience in the following conclusions:

1. That in all the cases in which the drug was given continuously there was noted a reduction in the number of the fits, ranging from about 25 to 75 per cent. as compared with other months during which patients were on bromide and tonic treatment.

2. The remedy was in all cases well borne, producing no apparent mental or physical depression. This is in marked contrast with the depressant effects noted after a course of bromide treatment.

3. No eruption of the skin was produced.

4. In any case where a great number of fits are occurring, and in which it is desirable to control them as soon as possible, the bromides would be of far more value than antifebrin.

A CASE OF TUMOR OF THE CAUDA EQUINA, ILLUSTRATING CERTAIN POINTS IN SPINAL LOCALIZATION.

THORBURN (*Manchester Medical Chronicle*, June, 1889, 177) reports the following interesting case of tumor of the cauda equina: The patient, a man of thirty-one years of age, had no history of syphilis or alcoholism. Six years previously he had commenced to suffer pain in the lower part of the back, extending thence down the back part of the lower limbs to the heels. Coincidently with this he experienced difficulty in micturition at times, and obstinate constipation. This condition lasted until eight months before he came under observation, and may be called the first stage of the disease. The pain in the back of the legs was now gradually replaced by numbness, and pain developed in the front of the thighs, from the hips to the knees. At the same time the patient noticed feebleness in the lower limbs, especially on the right side. After this there came loss of the power of coitus, though with occasional erections and nocturnal emissions. This condition of affairs, which lasted until three months before entering into the hospital, the author calls the second stage of the disease.

For the last three months the pain, numbness, and muscular weakness had grown worse, and he had ceased to have erections. This was the third stage of the affection.

On admission to the hospital, the lower limbs were wasted; there was no paralysis, though all the muscles were weak, and the toes showed a tendency to drop in walking. Muscle sense and the electrical reactions were normal; the bowels obstinately constipated, and the urine absolutely retained. The sensory affections were most interesting. The lower lumbar vertebræ in the middle line were painful on pressure and on movement. There was pain over the front of the thighs, but no exaggeration of sensibility to pain on touch. There was nowhere absolute anæsthesia, but great blunting of sensation in the penis, scrotum (except at the upper part), perineum, gluteal region, and antero-posterior aspects of the upper parts of the thighs. This blunting extended down the centre of the back of the thighs, over the calves (except on the inner side), and to the soles of the feet, where it was again more developed, but spared the inner aspect of the foot, great toe, and half the next toe. Less marked loss of sensation affected the front of the knees, the outer side of the legs, and the outer side of the dorsum of the foot. The sensory deficiency was more marked in the leg and foot of the right side than of the left. Some sensation was retained in the urethra.

The plantar and gluteal reflexes were lost on both sides; the cremasteric reflexes were retained, and the abdominal exaggeration; there was no ankle clonus; knee-jerk was absent on the right side; and, though present on admission, soon disappeared on the left side.

The author thinks there is no doubt that the patient was suffering from pressure on the cauda equina. The progress of events was too slow for us to suppose that a disease of the lumbar cord existed. The duration of the case and the course of the symptoms pointed toward tumor rather than meningitis. The history of the patient contra-indicated tubercle, and the growth was, therefore, probably a connective-tissue tumor.

In pressure lesions of the cauda equina the lower roots almost invariably suffer most. Consequently, there was first in this case paralysis of the fourth sacral root, which supplies the bladder and rectum, together with irritation, causing pain in the sensory branches of the first three sacral roots. In the second stage, coming on eight months after admission, the third sacral root was also paralyzed, with resulting loss of power of coitus; and at the same time anæsthesia developed in the previously painful areas; while the irritation spread to the third and fourth lumbar roots.

In the third stage, coming five months later, pressure on the second sacral root produced paralysis of the *nervi erigentes*. Anæsthesia was now well marked in the cutaneous distribution of all the sacral roots, and was less obvious in that of the fifth lumbar; pain being confined to the distribution of the third (? and fourth) lumbar roots.

Motor weakness affected most of these roots, but was well marked in the second sacral (supplying the extensors of the toes, etc.), and was still greater in the third and fourth sacral. The first two lumbar roots escaped entirely.

The tumor must have been situated below the upper border of the second lumbar vertebra, because the spinal cord terminates here, and it is clear the cord was not involved. As the third lumbar roots were certainly affected, the growth must have been situated between the upper border of the second and the lower border of the third lumbar vertebræ.

ORIGIN OF THE VESICULAR RESPIRATORY MURMUR.

DEHIO (*Deutsche med. Wochenschr.*, No. 29, 595, 1889) claims that several clinical facts oppose the explanation given by Boas and Penzoldt of the origin of the vesicular respiratory murmur, according to which it is to be considered as a bronchial respiration, a stenosis-sound, produced in the larynx and modified by passing through the pulmonary parenchyma containing air. Among these facts is the diminution of the vesicular murmur in cases of laryngeal stenosis, though there is a very loud sound of stenosis produced in the larynx. The diminution of the murmur in dry pleurisy, owing to the limitation of motion of the affected side as a result of pain, and the harsh puerile breathing of children, though without any increase of bronchial breathing over the larynx and trachea, are both opposed to the theory of Penzoldt. If heavy weights are laid on one side of the thorax of a man in a reclining position, until the respiratory movements are nearly or entirely prevented, the vesicular murmur is made decidedly weaker than on the other side, which could hardly be the case were this simply a transmitted sound. Dehio holds that vesicular respiration is not a sound transmitted from the larynx, but is produced within the lungs, and he believes that the following experiment demonstrates this: A lung, dried by injections of glycerine and consequently elastic, is distended by air blown from bellows through the bronchus; but at the same time a loose plug of cotton placed in the bronchus does away with any bronchial murmur which might otherwise arise. The ear placed over the lung will now hear no bronchial murmur, but a loud and unmistakable vesicular murmur. We must, therefore, return to the former view, that the vesicular sound is produced within the lung.

Regarding the question as to how this is brought about, there are two possibilities: 1. That the sound is due to vibrations of the air passing through the lungs, these vibrations being seated in the finest bronchioles or in the alveoli; since were they produced in the large bronchi, a bronchial murmur would be the result. 2. The view of Gerhardt, that the vesicular sound is the result of the tension on the pulmonary parenchyma, especially of the alveolar septa, occurring at each inspiration. Dehio believes the following experiment proves that Gerhardt's explanation is not correct, and that the theory of the vibrations of the current of air is the one to be adopted. The glycerine-dried lung is moderately distended, the cotton placed in the bronchus, as before, and for the same purpose, and the bronchus then closed entirely air-tight. If now gentle compression be made on any part of the lung and then the pressure be relaxed, a distinct vesicular murmur can be heard on auscultation. This must be due to the motion of the air from one part of the parenchyma to another under the influence of the pressure, and the return of the same to the compressed alveoli after the pressure is removed. With the moderate distention of the lung, there can be no possibility of tension on the parenchyma such as Gerhardt claims. Since we know, now, that sonorous vibrations of air in the bronchial tubes can only be produced where there is a sudden alteration of calibre, the sound must be developed at the point at which the finest bronchi normally undergo a sudden narrowing—*i. e.*, at their last bifurcation, and at the point of entrance of the bronchi into the infundibula. The combination of these numerous stenosis-murmurs is the origin of that acoustic phenomenon which we designate the vesicular respiratory murmur. In the living person, however, there is a combination of this with the bronchial sound transmitted from the trachea and bronchi, and altered by passing through lung tissue. The vesicular murmur heard on the living chest is, therefore, somewhat modified, and is not always so distinct as that heard over the prepared lung.

TWO CASES OF ANEURISM OF THE ARCH OF THE AORTA PERFORATING INTO THE SUPERIOR VENA CAVA.

W. T. GAIRDNER (*Lancet*, 1889, i. 1233) reports a case of this rare affection. The patient, aged forty-four years, exhibited very marked cyanosis with general anasarcous swelling of the upper part of the body, extending secondarily down the back as far as the sacrum, but not at any time affecting the lower extremities. He was under observation in the Glasgow Western Infirmary for a week before death took place. The patient had for some years experienced a slight degree of pulsation in the neighborhood of the right sterno-clavicular articulation, which did not, however, give him any concern. A week before he came to the hospital he had a sensation as if "something had given way" to the left of the cardiac apex. This was accompanied by faintness, but not by pain. Swelling of the face and right arm followed. There was some dilatation of the superficial veins all over the front, especially on the lower sternal region. The veins of the hands were also distended; more markedly on the left side. A loud double murmur, having the distribution of that of double aortic disease, could be heard all over the front of the chest. The pulse at the right wrist was stronger than at the left, and exhibited here the usual characters of the pulse of unfilled arteries. The pupils were equal. There was some displacement of the præcordial dulness downward and to the left, and another area of dulness beneath the manubrium extending both to the right and left. In spite of the cyanosis there was but little cough or dyspnœa, but a faint tracheal quality of the respiratory murmur could be heard at the upper sternum. Stertor developed before death, but was probably largely cerebral.

The autopsy revealed a general dilatation of the transverse portion of the arch, but with two more localized dilatations, one extending to the right, the other to the left. The first of these compressed the *vena cava superior*, and was adherent to it, forming a round bulging into the interior of the vein, corresponding with the middle and upper third of this vessel. On the summit of the bulging there was a rounded aperture of communication a quarter of an inch in diameter, and an inch and three-quarters beneath the origin of the innominate vein.

Another instance of varicose aneurism of the aorta and superior vena cava is reported by R. SISLEY (*Lancet*, 1889, i. 1184). The patient, a man, thirty-five years of age, laborer, had but an indistinct history of syphilis, and was not intemperate. For some months he had had a cough, but did not complain of shortness of breath. His eyelids were puffy and his face somewhat swollen on returning from work in the evenings. This condition was only seen at night, and had always disappeared by the morning. Upon one occasion he did not feel as well as usual, and stopped at St. George's Hospital for advice; but he did not consider himself seriously ill, and wished to go home. His face and neck were at this time deeply cyanosed, his eyelids puffy, and his ears almost black. His arms were slightly cyanosed, but the other parts of his body were of a natural color. A double murmur was heard over the middle of the sternum. His mental state was clear, but he was drowsy. He complained of no pain and had no paralysis. The cephalic and jugular veins pulsated.

The diagnosis of an aortic aneurism pressing on the superior vena cava was made. The patient was bled to sixteen ounces, but without relief, and there was considerable difficulty experienced in stopping the hemorrhage. His face and arms became gradually more cyanosed, and he became more drowsy, and finally comatose, and died in about two hours after admission to the hospital.

The autopsy revealed a saccular aneurism three-quarters of an inch above the posterior leaflet of the aortic valve. The opening into the aorta was of a rounded form and one and a quarter inches in diameter. Beyond the opening the sac measured about one and a half inches in its transverse diameter, and two inches from above downward. The aneurism extended between the walls of the aorta and the right ventricle as far downward as the interventricular septum, and upward to a point three-quarters of an inch below the opening of the innominate artery. About the centre of the right posterior part of the aneurism there was an opening into the vena cava immediately above the entrance of that vessel into the right auricle. The opening was of an irregular form, while its edges were ragged and thin, and projecting slightly into the vena cava. The sac contained a small, non-laminated clot, not adherent to the walls.

The author believes that the œdema of the face which the patient often had after a hard day's work, was due to the pressure of the aneurism on the superior vena cava. The final catastrophe was caused by the rupture of the aneurism. He makes a very brief allusion to other instances of this form of varicose aneurism. He accounts for its rarity on anatomical grounds, viz., that to produce it, two layers of pericardium have to be broken through, and adhesions must have been previously formed between the layer covering the aorta and that covering the vena cava, else the rupture would be intrapericardial.

A NEW METHOD FOR THE APPROXIMATE QUANTITATIVE ESTIMATION OF ALBUMIN IN THE URINE.

A. CHRISTENSEN (*Centralbl. f. klin. Med.*, No. 30, 523, 1889, from *Virch. Arch.*, cxv. H. 1) recommends the following procedure: The urine containing albumin is precipitated with a dilute solution of tannic acid, and the precipitate thus obtained suspended in a definite quantity of water, by the aid of a small amount of a solution of gum arabic. In this way a quite permanent homogeneous emulsion is produced.

To find the quantity of albumin in the emulsion, we determine the degree of its opacity. For this purpose there are required (1) a burette, and (2) a cylindrical glass of exactly four centimetres in diameter. This latter is placed over a piece of white paper, on which a series of black lines, each of a definite breadth, have been made at a definite distance from each other. After the emulsion has been diluted to a certain extent it is poured from the burette into the glass until the lines on the paper can no longer be distinguished from each other. The albumin present is calculated from the quantity of fluid which it was necessary to employ. The author has tested this method and finds it quite accurate as well as practical. It has been found, however, that there is a certain amount of error on account of differences in the intensity of the light and in the individual ability of the observers to distinguish the lines.

SAFRANINE—A NEW REAGENT FOR SUGAR.

GRISMER (*Annales de Soc. Méd. Chir. de Liège*, May, 1889, quoted in *Manchester Med. Chronicle*, x. No. 3, 211, 1889) recommends that as a test for sugar 1 cc. of urine be boiled with 5 cc. of a solution of safranine (1:1000) and 2 cc. of a solution of caustic potash. If decolorization be detected, the urine is diabetic; normal urine containing too little sugar to decolorize completely so much of the reagent. On cooling the solution becomes turbid. The author considers this reagent superior to Fehling's solution, because it is not decolorized by uric acid, creatinin, chloral, chloroform, peroxide of hydrogen, or the salts of hydroxylamine, all of which reduce the Fehling's fluid, and thus lead to an error in diagnosis. It is true that chloral and chloroform attenuate the red color of the safranine, but never to a complete decolorization. On the other hand, albumin decolorizes it wholly, but only after some length of time. Occasionally normal urine will also do this, but the author believes that it then contains a little sugar, for on submitting it to the fermentation test such a specimen will show bubbles of carbonic acid gas rising to the surface, no further decolorization being obtainable on adding more of the reagent.

THE DETECTION OF GONOCOCCI.

J. SCHUETZ (*Münch. med. Wochenschr.*, xxxvi. No. 14, 235, 1889) describes the methods hitherto employed for the detection of gonococci in the blenorrhagic secretion, as consisting simply in the action of an aqueous aniline color, and the removal of the excess of the dye with water. Only in the method of Eschbaum is alcohol used for the differentiation, and even here the nuclei and other portions of the cell remain so deeply colored that the discovery of the microbes, when present only in small numbers, is a difficult matter. The attempts to produce an isolated coloring of the gonococci have failed on account of the fact that these cocci, in contradistinction to other varieties, are decolorized by most acids and by the Gram method. As, however, the gonococcus is not affected by acetic acid to so great a degree, the author recommends the following method as simple and effectual:

The carefully prepared cover-glass preparations are placed for from five to ten minutes in a cold, filtered, saturated solution of methylene blue in five per cent. carbolic-acid water. They are then washed in water, and afterward placed for a moment (as long as it takes to count one, two, three, slowly) in a solution of five drops of acetic acid in twenty cubic centimetres of distilled water, and at once again washed thoroughly in pure water. Everything in the preparation is now decolorized except the gonococci, which remain distinctly blue. It is now well to double-stain the preparation with a very dilute watery solution of safranine. The action of this color must be slight only, and be at once interrupted as soon as the cover-glass, after being washed in water, shows a trace of the safranine color when held over a white surface. Under the microscope the safranine will be distinct enough. These preparations may be permanently mounted in Canada balsam, which has been warmed on the slide without the addition of any solvent. The gonococci will be found deep blue, and the epithelial cells of the same color; while the pus cells and their nuclei are salmon-colored.

SURGERY.

UNDER THE CHARGE OF

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TRANSFUSION.

DR. WILLIAM HUNTER (*The British Medical Journal*, July 27, Aug. 3, and Aug. 10, 1889) publishes an elaborate experimental and critical study of the value of the different forms of transfusion. The paper is by far the most valuable recent contribution to this subject, and well deserves careful study. His main conclusions as to the practice of transfusion, and the reasons therefor, are:

1. Transfused blood possesses no nutritive value. Transfusion of blood is, therefore, useless in all forms of "atrophic" anæmia, by which term, rather than by the unfortunate term "symptomatic," hitherto given, he would distinguish all forms of anæmia in which the changes in the blood simply correspond in degree to the wasting changes occurring in other tissues.

2. If the condition to be met with is a want of red corpuscles, or an incapacity on the part of those already present to carry on their respiratory functions, then transfusion of blood may possibly be of value. The red corpuscles transfused remain for a certain time within the circulation of their host. Transfusion of blood may conceivably be of value in carbonic oxide poisoning, where the red corpuscles are incapacitated for their work by a close union of the gas with the hæmoglobin of the corpuscles.

3. If the condition to be met with is a failure in blood formation, due, perhaps, to the want of iron in the system, transfusion of blood may be of some value, in so far as we in this way supply the organism with iron in the form in which it can most conveniently be stored up in the body. The two